a vacuum the phenomena observed by Savart do not take place, all kinds of powder collecting at the nodes. In the investigation of this, as of the other problems, the motion is supposed to take place in two dimensions.

It is probable that the colour phenomena observed by Sedley Taylor* on liquid films under the action of sonorous vibrations are to be referred to the operation of the aerial vortices here investigated. In a memoir on the colours of the soap-bubble,† Brewster has described the peculiar arrangements of colour accompanied by whirling motions caused by the impact of a gentle current of air. In Mr. Taylor's experiments the film probably divides itself into vibrating sections, associated with which will be aerial vortices reacting laterally upon the film.

The third problem relates to the air-currents observed by Dvorak in a Kundt's tube, to which is apparently due the formation of the dust figures. In this case we are obliged to take into account the compressibility of the fluid.

VII. "The Influence of Bodily Labour upon the Discharge of Nitrogen." By W. North, B.A., F.C.S. Communicated by Professor J. S. Burdon Sanderson, F.R.S. Received October 29, 1883.

(Abstract.)

The scope of this inquiry has been strictly limited to one question, viz., that of the influence of labour in modifying the normal relation between food and excreta. No attempt has been made to investigate the *mode* in which nitrogenous products come into existence in the organism.

The researches immediately bearing on the subject of this paper are those of Dr. Parkes ("Proc. Roy. Soc.," vols. 16 and 21), and those of Dr. Austin Flint, made on the pedestrian Weston ("New York Med. Journal," June, 1871). Dr. Parkes found that bodily exercise caused a slight increase in the nitrogen discharge during or immediately after labour. The increase was, however, so inconsiderable that it may well be questioned whether it could not be accounted for as dependent on the more perfect absorption of food; for although the diet of the soldiers experimented upon was carefully regulated, and the nitrogen it contained determined by analysis, with the result that before work the quantity of nitrogen taken in considerably exceeded the quantity discharged, the two became practically equal during the work period. Consequently if the whole period of observation is

* "Proc. Roy. Soc.," 1878. † "Edinburgh Transactions," 1866-67.

taken into account, the nitrogen discharged is found to be more than balanced by that of the food.

Dr. Austin Flint, on the other hand, found that over the whole period of work the excess of discharge was so large that no such explanation appeared to him admissible. If, however, comparison be made of the intake with the output of nitrogen during the whole time of observation, comprising three periods of five days each, before, during, and after labour, it is found that the two are unequal, the difference in *favour* of the nitrogen of the food amounting to 217 grs. in a total of 5075.

These results were subjected to careful experimental criticism in 1876 by Dr. Pavy ("Lancet," 1876, vols. i and ii), who showed as the result of his own analyses that the immediate effect of labour in increasing the nitrogen output is more than compensated by the concomitant and subsequent intake.

It is further to be considered that whatever results had been obtained by Dr. Austin Flint, they could not have been received without some misgiving, for his methods of research were insufficient as bases for quantitative statement. Thus the nitrogen of the *urine* was throughout determined by a process which is known to be liable to errors of variable amount, and which no care on the part of the worker is adequate to guard against.

So also as regards the intake of nitrogen. Dr. Flint's estimates were founded for the most part not on actual analyses of the material used, but on calculations based on the percentages given in M. Payen's tables ("Traité des Substances Alimentaires," 1865), which are known to be at best only approximately correct; moreover, the diet of Weston was of so complicated and variable a composition that, even if each constituent had been analysed, the result would still have been open to question.

The circumstances under which Dr. Austin Flint had to make his observations, probably made it impossible for him to secure uniformity of diet. In this respect the conditions of Dr. Parkes' experiments were immeasurably superior. Fully recognising that uniformity was essential, he fed his men in the simplest possible way; he was not, however, able to accomplish this without employing a diet which was so different from that to which, as soldiers, they were accustomed, that, however satisfactory its elementary composition might be, it could scarcely be considered as natural.

Notwithstanding this difficulty, the experiments of Dr. Parkes render it, to say the least, highly probable that the immediate effect of labour is to increase the discharge of nitrogen; they leave it undecided whether or not this increase occurs at the expense of stored material independently of any concomitant or subsequent increase of intake.

The decision of this point is the *object* of my experiments.

It will be obvious from the consideration of the experiments above mentioned, that in approaching this question, methods of research are of prime importance. These divide themselves very naturally under two heads—first, the diet, and secondly, the investigation of the excreta in order that the results may be satisfactory. It is essential first that the daily intake of nitrogen should be accurately known and admit of exact regulation, and secondly that the mode of analysis of the excreta should not be open to question.

The use of the method of combustion with soda-lime effectually disposes of the latter difficulty; the former is not so readily overcome.

It is a matter of common knowledge that the composition of the ordinary food-stuffs in their usual state is, from the chemist's point of view, exceedingly variable. The first consideration then is how to reduce them to such a state that this objection shall disappear.

The mode in which I have overcome this difficulty constitutes the chief difference between my experiments and those of other observers.

It would occupy too much space, and is indeed unnecessary, to describe in detail the process of preparation of the food-stuffs, and it will here suffice to say that I have acted on the principle that only fluids or powders can be accurately sampled and analysed; all my food-stuffs have been reduced to one of these states, and I may here enumerate them—

Meat (dried and ground to powder).
Flour.
Vegetables (dried).
Potato (Edward's patent desiccated).
Condensed milk.

All these articles can be obtained in quantity; all admit of being easily and accurately weighed and measured; they are in a state which readily admits of accurate analysis, and will keep indefinitely. Further they are the constituents of an ordinary mixed diet, and except in their palatability and mode of preparation, involve no serious departure from one's usual food.

By the use of these materials I can, knowing their composition, adjust the intake of nitrogen with the greatest possible nicety, and maintain it at any desired level for almost any length of time, besides having it in my power to use, not merely a similar, but precisely the same, diet at any time when it may seem desirable to repeat an experiment.

My plan of experiment was as follows:—

Believing as I do that previous food may materially affect the results of an experiment, and that the body has the power of storing nitrogen at one time which can be discharged at another as occasion

requires, I regulated my diet for some four or five days before beginning an experiment, and took rather more exercise than usual in order to get rid of any possible surplus in the body; in the latest experiments I have adopted the plan of abstaining from food on the first day of an experiment, in order to effect the discharge of this surplus, and I am inclined to regard this as the best method to pursue.

I then place myself upon a regulated diet of accurately known composition for nine or ten days, and about the middle of the period I perform a certain amount of muscular labour. In all experiments made thus far the labour consisted in walking a known distance, and carrying a load whose weight was accurately determined. By observing the relation of the nitrogenous intake to the output from day to day, I was enabled to determine with very considerable accuracy the effect produced by the labour upon it. In addition to the nitrogen the phosphoric and sulphuric acids were accurately determined in the food and excreta, and so I was able to institute a comparison, and observe the effect of the exercise in modifying the relationship.

It is impossible in a brief abstract to enter into the immense mass of detail which these experiments involve, but the following tables will, I think, suffice to exhibit the general nature of the results obtained.

Experiment I, 1882.

	Whole experiment.	Daily.
Nitrogen of urine	135·33	15·03
,, fæces	20·69	2·29
Total excreta	156·02	17·32
Total ingesta	158·78	17·64
Difference	2·76	0·32
P_2O_5 of urine	18·00	2·00
P_2O_5 of fæces	19·44	2·16
Total excreta	37·44	4·16
Total ingesta	34·84	3·87
Difference	2·60	0·29

Daily.	Before work.	After work.	Difference.
Nitrogen of urine	14·15	15·74	+1·59
,, fæces	2·48	2·15	−0·33
Total nitrogen	16·66	17·89	+1·23
P_2O_5 of urine	2·01	2·00	−0·01
P_2O_5 of fæces	2·54	1·85	−0·69
Total P_2O_5	4·55	3·84	−0·71
H_2SO_4 in urine	2·76	3 00	+0·24

Experiment II, 1882.

	Whole experiment.	Daily.
Nitrogen of urine	131·60	14·62
" fæces	19·05	2·11
Total excreta	150·65	16·74
Total ingesta	158·78	17·64
Difference	8·13	0·90
P_2O_5 of urine	17·07	1·89
P_2O_5 of fæces	18·27	2·03
Total excreta	35·34	3·92
Total ingesta	34·84	3·87
Difference	0·50	0·05

Daily.	Before work.	After work.	Difference.
Nitrogen of urine	13·77	15·29	+1·72
" fæces	1·19	2·65	+1·46
Total excreta	15·22	17·95	+2·73
P_2O_5 of urine	1·97	1·83	−0·14
P_2O_5 of fæces	1·62	2·35	+0·73
Total excreta	3·59	4·19	+0·60
H_2SO_4 in urine	2·74	2·97	+0·23

Experiment III, 1882.

This experiment was begun and concluded by a fast of twenty-four hours; two sets of totals are therefore given, one including the first fast and one not including it.

	12 days.	Daily.	11 days.	Daily.
Nitrogen of urine	175·02	14·58	165·56	15·05
" fæces	22·80	1·90	22·80	2·07
Total excreta	197·82	16·48	188·36	17·12
Total ingesta	194·06	16·17	194·06	17·64
Difference	3·76	0·31	6·30	0·52
P_2O_5 of urine	21·99	1·83	20·84	1·89
P_2O_5 of fæces	20·51	1·70	20·51	1·86
Total excreta	42·50	3·54	41·35	3·75
Total ingesta	42·59	3·55	42·59	3·87
Difference	0·09	0·00	1·24	0·12

Two days of work were introduced in this experiment, it is therefore to be considered as two experiments in one.

Experiment III, 1882.

1st Period.

Daily.	Before work.	After work.	Difference.
Nitrogen of urine	12·92	13·47	+0·55
" fæces	1·43	2·14	+0·71
Total nitrogen	14·35	15·61	+1·26
P_2O_5 of urine	1·80	1·51	−0·39
P_2O_5 of fæces	0·95	2·01	+1·06
Total P_2O_5	2·75	3·52	+0·77
H_2SO_4 of urine	2·58	2·83	+0·25

2nd Period.

Daily.	Before work.	After work.	Difference.
Nitrogen of urine	13·23	18·27	+5·04
" fæces	1·83	2·43	+0·60
Total nitrogen	15·07	20·75	+5·68
P_2O_5 of urine	1·63	2·35	+0·72
P_2O_5 of fæces	1·56	2·40	+0·84
Total P_2O_5	3·19	4·75	+1·56
H_2SO_4 of urine	2·72	3·40	+0·68

The 2nd Period.

The whole of Period I is included in the time before work, although work was done.

The work done.

Experiment.	Distance walked.	Miles per hour.
I, 1882	30 miles	4·28
II, 1882	32 "	4·57
III, 1882—		
Period I	33 "	4·71
Period II	47 "	4·7

In each case the load carried was about 27 lbs.

Conclusions.

My results, while they confirm those of Dr. Parkes, show that the disturbance produced by very severe labour is much more immediate and of much greater intensity than that which Dr. Parkes observed, the explanation obviously being that in his experiments the exertion imposed on the soldiers who were made the subjects was inadequate.

I have further been able to show as the result of a very lengthy experiment in which several days of complete abstinence from food were introduced, and of which it is impossible in a short abstract to give the figures, that just as in Dr. Parkes' experiment on the effect of privation of nitrogenous food the diminution of the nitrogen stored in the system was followed by retention, *i.e.*, by a state of things in which the intake was greater than the output, so, after the disturbance of the nutrition of the body which is produced by severe labour, the immediate effect of which is to diminish the store of nitrogenous material in the system, there follows a corresponding diminution of discharge; so that the result is the same, viz., to use Dr. Parkes' own words "an insufficient supply at one time must be subsequently compensated" *whether the insufficiency be due to privation of food or to exercise.*

A third result of importance is this: that this storage of nitrogen is the expression of a tendency of the organism to economise its resources, which is much more constantly operative than has hitherto been supposed.

Finally, as regards the phosphates and sulphates, it has been shown that unless the exertion be very severe the phosphates are not increased, whilst the output of sulphates is distinctly increased in every case, the increase being in general terms proportional to that of nitrogenous material.

It is a matter of regret that the total sulphur of the food was not estimated; it is known that the percentage of sulphates contained in the food was insignificant as compared with that excreted in the urine, and consequently almost all the discharge must have been a product of oxidation.

I beg leave in conclusion, to state that the expenses of the present research, which have been extremely heavy, have been defrayed by a grant of the British Medical Association. I desire to express to the Association my most grateful thanks.